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## Comment on nhess-2022-192

Anonymous Referee #1

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Referee comment on "Heat wave monitoring over West African cities: uncertainties, characterization and recent trends" by Cedric Gacial Ngoungue Langue et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-192-RC1>, 2022

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### **Heat waves monitoring over West African cities: uncertainties, characterization and recent trends**

#### **General comments**

This manuscript assesses potential uncertainties encountered in the process of heat wave monitoring and analyse their recent trend in West African cities. This is investigated using downscaled ERA5 and MERRA variables for the period 1993-2020. Three types of uncertainties are discussed. The first type is related to the reanalyses themselves; the second, to the sensitivity of heat wave frequency to the threshold values used to define them; and, finally, the third is related to the choice of indicators and the methodology used to define heat waves.

#### **Specific comments**

- The abstract is rather long. It contains a surplus of information that could better fit in other sections of the paper such as the introduction. I might suggest keeping it more concise, only stating the main problems & objectives, how they have been dealt with and the main results obtained.
- The abstract does not mention that local stations have been used nor the downscaling methodology applied. As it is now it seems that it only uses reanalysis data, and this gives a sense of contradiction with the title (which emphasizes the application to cities).
- The analysis conducted to define the three areas is not provided (this is highlighted by the authors). Since this division is a core aspect of the paper, I think it is important to provide, at least, a description of the method followed to obtain these three areas (it can be included as Supplementary Material if the authors consider that it is too dense for the main document). Besides, the authors note that their interest in in the coastal

zone of West Africa region (lines 109). In that case, I am not sure why the analysis of a 'Continental zone' is needed. I would suggest to either rephrase the 'focus' on the coastal area or take out the continental zone analysis.

- In section two: Region of interest, Data and Methods, I would suggest the authors rearranging the contents to have only three subsections: 2.1 Region of interest; 2.2 Data and 2.3 Methods (with the corresponding sub-subsections). In 2.2 Data, for instance, I would suggest including the general information on the different reanalysis used (resolution, time-period, climate variables, etc.) as well as the information on the local station data (location, source, time-period, climate variables, percentage of missing values, quality of the series, etc.). Now it is not easy nor clear to find which local information has been used and its characteristics.
- Although the authors have clearly stated which are the uncertainties that have been studied in the paper, there is a general lack of justification why each number of choices is enough to characterize each uncertainty. Why using only ERA5 and MERRA, for example? There are other reanalyses. Perhaps is it ok to stay with these two, though. In any case, there is a need to better justify whether two are enough to 'characterize' (which implies some sort of specific quantification from a statistical point of view) or, conversely, if they can only be used to 'illustrate' the magnitude of the uncertainties can be important enough to affect the conclusions. The same applies to the uncertainties linked to thresholds and the different ways to define a heatwave. There is a need to better justify and discuss why the authors think their choice of methods and thresholds is enough to map the uncertainties and to which degree this could be achieved.
- There is a need to further explain the downscaling method applied as well as the need for it. The method is not clear, nor how is it applied, as well as which stations were used and why. If I'm not mistaken, the method is applied because the reanalysis products are not enough to go to the city level, and there are not enough stations in the cities of interest to just use point station data. If this is the case, this has to be better explained in the Methods subsection (and, possibly, in the introduction and conclusions sections, too). Hence, I would suggest the authors to expand this section or provide a more detailed description of the methodology as Supplementary material.
- The use of relative thresholds to establish heatwave duration for all the year, though it is systematic, implies that for some regions and periods, the 'heat waves' have different impacts. It could happen, that for some regions and periods, though formally there could be a heat wave, in practice, there would not be any impact at all from it. This needs to be highlighted and discussed, to justify that, in any case, the analysis for all the year it is still useful.
- When performing the comparison through statistical metrics, besides clearly stating that the data is downscaled, I would also suggest comparing the 'downscaled values' with the station ones (whenever possible). It would also be needed to specify why choosing ERA5 as the reference (instead of MERRA or any other station network).
- In the maps at the end, I would suggest using discrete colour bars (continuous ones are not suitable for assigning values). I would also include the cities of interest in all the maps (since this is the focus of the paper).
- Figure 4 depicts the slope of the linear regression in heatwaves (also figure 5). The caption says that the slope is computed using the 75<sup>th</sup>, 80<sup>th</sup>, 85<sup>th</sup> and 90<sup>th</sup> percentiles, but there is only one map per variable and reanalysis. Does this mean that the trend is computed for all the four percentiles simultaneously? This is not very clear when reading the methods section, and a rephrasing and/or extension of the description would be advisable. Besides, there is also a need to state more clearly (both in the methods and in the captions) which method has been applied to compute the significance of the slope. That said, if the slope is computed with all the four thresholds simultaneously, it wouldn't be a conventional approach and, consequently, a more thorough justification about its correctness and its utility would be needed (compared to performing the analysis independently for each threshold).

## **Technical corrections**

- Figure 6 lacks titles in the top row.
- Figures should include units when necessary. For example, in figure 1, 'meters above sea level; in figure 6 it would be 'number of days' or 'Number of events / occurrences'); in figure 3 and figure 4, number of events or days / year; figure 7; figure 11...
- It is not clear how figure 2 has been obtained. Is it built with data from all the stations? Cities? Grid-points? It is just an illustration for a single grid-point? This has to be included in the caption (as well as in the main text).
- Sometimes x- axis and y- axis is written in capital letters and sometimes it is not.
- The acronyms for variables should be the same in figures (titles, for instance), captions as well as in the main text.
- Column titles in figure 8 and 9 are difficult to understand. Besides, the idea to display different parameters in the same format it is confusing (apparently, from the caption, 2<sup>nd</sup>, 3<sup>rd</sup> and 4<sup>th</sup> columns display percentages instead of duration of heatwaves). I would suggest to only maintain the same format when displaying the same elements.
- In figure 10 it is not clear what those percentages refer to. Are percentages from the total of days? From the total of heat wave days? Do they have to sum 1 in total? The phrase 'using maximum values of indicators based on the duration' is not very clear, either. What does this refer to? The thresholds? The methods? The variables? This also extends to the other figures applying the same approach.